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VIII. *On the noxious Quality of the Effluvia of putrid Marshes. A Letter from the Rev. Dr. Priestley to Sir John Pringle.*

TO SIR JOHN PRINGLE, Baronet.

DEAR SIR,

Redd, Dec. 16, 1773. **H**AVING pursued my experiments on different kinds of air considerably farther, in several respects, than I had done, when I presented the last account of them to the Royal Society; and being encouraged by the favourable notice which the Society has been pleased to take of them, I shall continue my communications on this subject; but, without waiting for the result of a variety of processes, which I have now going on, or of other experiments, which I propose to make; I shall, from time to time, communicate such detached articles, as I shall have given the most attention to, and with respect to which, I shall have been the most successful in my inquiries.

Since the publication of my papers, I have read two treatises, written by Dr. ALEXANDER, of Edinburgh, and am exceedingly pleased with the spirit
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of philosophical inquiry, which they discover. They appear to me to contain many new, curious, and valuable observations; but one of the conclusions, which he draws from his experiments, I am satisfied, from my own observations, is ill founded, and from the nature of it, must be dangerous. I mean his maintaining, that there is nothing to be apprehended from the neighbourhood of putrid marshes.

I was particularly surprized, to meet with such an opinion as this, in a book inscribed to yourself, who have so clearly explained the great mischief of such a situation, in your excellent treatise on the diseases of the army. On this account, I have thought it not improper, to address to you the following observations and experiments, which I think clearly demonstrate the fallacy of Dr. ALEXANDER's reasoning, indisputably establish your doctrine, and indeed justify the apprehensions of all mankind in this case.

I think it probable enough, that putrid matter, as Dr. ALEXANDER has endeavoured to prove, will preserve other substances from putrefaction; because, being already saturated with the putrid effluvia, they cannot readily take any more; but Dr. ALEXANDER was not aware, that air thus loaded with putrid effluvia is exceedingly noxious when taken into the lungs. I have lately, however, had an opportunity of fully ascertaining how very noxious such air is.

Happening to use at Calne, a much larger trough of water, for the purpose of my experiments, than I had done at Leeds, and not having fresh water

so near at hand as I had there, I neglected to change it, till it turned black, and became offensive, but by no means to such a degree, as to deter me from making use of it. In this state of the water, I observed bubbles of air to rise from it, and especially in one place, to which some shelves, that I had in it, directed them; and having set an inverted glass vessel to catch them, in a few days I collected a considerable quantity of this air, which issued spontaneously from the putrid water; and, putting nitrous air to it, I found that no change of colour or diminution ensued, so that it must have been, in the highest degree, noxious. I repeated the same experiment several times afterwards, and always with the same result.

After this, I had the curiosity to try how wholesome air would be affected by agitation in this water; when, to my real surprise, I found, that after one minute only, a candle would not burn in it; and, after three or four minutes, it was in the same state with the air, which had issued spontaneously from the same water.

I also found, that common air, confined in a glass vessel, in contact only with this water, and without any agitation, would not admit a candle to burn in it after two days.

These facts certainly demonstrate, that air which either arises from stagnant and putrid water, or which has been for some time in contact with it, must be very unfit for respiration; and yet Dr. ALEXANDER's opinion is rendered so plausible by his experiments, that it is very possible that many persons may be rendered secure, and thoughtless

of danger, in a situation in which they must necessarily breathe it. On this account, I have thought it right to make this communication as early as I conveniently could ; and as Dr. ALEXANDER appears to be an ingenuous and benevolent man, I doubt not but he will thank me for it.

That air issuing from water, or rather from the soft earth, or mud, at the bottom of pits containing water, is not always unwholesome, I have also had an opportunity of ascertaining. Taking a walk, about two years ago, in the neighbourhood of Wakefield, in Yorkshire, I observed bubbles of air to arise, in remarkably great plenty, from a small pool of water, which, upon inquiry, I was informed had been the place, where some persons had been boring the ground, in order to find coal. These bubbles of air having excited my curiosity, I presently returned, with a basin, and other vessels proper for my purpose, and having stirred the mud with a long stick, I soon got about a pint of this air ; and, examining it, found it to be good common air ; at least a candle burned in it very well. I had not then discovered the method of ascertaining the goodness of common air, by a mixture of nitrous air. Previous to the trial, I had suspected that this air would have been found to be inflammable.

I shall conclude this letter with observing, that I have found a remarkable difference in different kinds of water, with respect to their effect on common air agitated in them, and which I am not yet able to account for. If I agitate common air in the water of a deep well, near my house in
Calne.

Calne, which is hard, but clear and sweet, a candle will not burn in it after three minutes. The same is the case with the rain water, which I get from the roof of my house. But in distilled water, or the water of a spring-well near the house, I must agitate the air about 20 minutes, before it will be so much injured. It may be worth while, to make farther experiments, with respect to this property of water.

In consequence of using the rain water, and the well water above-mentioned, I was very near concluding, contrary to what I have asserted in my printed papers, that common air suffers a decomposition by great rarefaction. For when I had collected a considerable quantity of air, which had been rarefied about four hundred times, by an excellent pump made for me by Mr. SMEATON, I always found, that when I filled my receivers with the water above-mentioned, though I did it so gradually as to occasion as little agitation as possible, a candle would not burn in the air that remained in them. But when I used distilled water, or fresh spring water, I undeceived myself.

I think myself honoured by the attention, which, from the first, you have given to my experiments, and am, with the greatest respect,

Dear Sir,

Your most obliged

humble Servant,

London, 7 Dec. 1773.

J. PRIESTLEY.

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POSTSCRIPT.

I cannot help expressing my surprize, that so clear and intelligible an account, of Mr. SMEATON's air-pump, should have been before the public so long, as ever since the publication of the forty-seventh volume of the Philosophical Transactions, and yet that none of our philosophical instrument makers should attempt the construction. The superiority of this pump, to any that are made upon the common plan, is, indeed, prodigious. Few of them will rarefy more than 100 times, and, in a general way, not more than 60 or 70 times; whereas this instrument must be in a poor state indeed, if it do not rarefy 200 or 300 times; and when it is in good order, it will go as far as 1000 times, and sometimes even much farther than that; besides, this instrument is worked with much more ease, than a common air-pump, and either exhausts or condenses at pleasure. In short, to a person engaged in philosophical pursuits, this instrument is an invaluable acquisition. I shall have occasion to recite some experiments, which I could not have made, and which, indeed, I should hardly have dared to attempt, if I had not been possessed of such an air-pump as this. It is much to be wished, that some person of spirit in the trade would attempt the construction of an instrument, which would do great credit to himself, as well as be of eminent service to philosophy.